Duff, James 1995

Dr. James Duff Oral History 1995

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James Duff
Interview date: March 23, 1995

This is an interview of Dr. James Duff, who played a key role in the Viruses Cancer Program, taken on March 23, 1995. The interviewer is Dr. Carl G. Baker, former Director of the National Cancer Institute.

Baker: Jim, perhaps first you could give us a little bit of background and tell us a little bit of your training and your experience before we get to the questions that I sent you beforehand?

Duff: Well, I started my career in microbiology at Ft. Detrick in 1949.

Baker: Your degree was taken where?

Duff: My degree was taken at Ohio State University.

Baker: In molecular biology?

Duff: In bacteriology, in those days. Then, in 1956, I left Ft. Detrick for 3 years and went to the University of Texas at Austin, where I was working with Dr. Orville Weiss, and returned to Ft. Detrick in 1959-1960. I got my Ph.D. at Austin and, I believe by that time, it was microbiology. I knew Dr. Stevenson when I was at Ohio State. He was at Ohio State when I was there. And when I got my Master's, I believe he was going for his Ph. D. And it was approximately 1965 when I heard from someone up at Ft. Detrick, probably Dr. Arnold Wedum, that they were starting this Special Virus Leukemia Program at the NCI. And the Public Health Service officer asked me if I knew of anybody who would like to go down there to work. And, after about two seconds, why, I raised my hand and said it was me. And, to make a long story short, I got hired at NCI in July of 1965, and went to work for Bob Stevenson which, at that time, was the Virology Research Reagents Branch, and I was involved, more or less, with the Testing and Monitoring Section, or segment, of the SVLP. The reason I was hired was to start a project out at Flow Laboratories where approximately 3 or 4 of us would be located. Approximately 6 months earlier than that, a project had been started by Chuck Boone out at Meloy Laboratories, and it was sort of a new arrangement where NCI employees would be at a contractor facility. And at our facility out at Flow, Dick Heberling, Jerry Kern, and eventually Barry Tampar and myself were the four NCI employees, and we were setting up a virus identification laboratory for the Special Virus Leukemia Program where anyone who came upon a virus that they wanted identified could send it in to the Virus Leukemia Program and have it identified. Initially, there was the need for a lot of reagent preparation and everything along that line was pretty well accomplished.

Baker: What kind of rate of submittal of viruses for identification was there at that time?

Duff: I would say that it was negligible at that time. By the time that we had gotten all the equipment and got everything all set up and were working along this line, Dr. Stevenson left NCI. I believe that was approximately when you became the Scientific Director for Etiology, which I think was in 1967. Is that about right?

Baker: That's right.

Duff: Right. At that time everything came to a screeching halt for me because it became evident that NCI was going to transfer Dr. Huebner from NIAID over to NCI and that I would be the liaison between NIAID and NCI for this transfer.

Baker: You got selected for that job partly because of your pleasant personality and you getting along with people.

Duff: As Dr. Rauscher said, he didn't know who else would do it, if I didn't do it. So, from that point on there was quite a change. I think there was an administrative change because Dr. Rauscher was now the Associate Director and was in charge of the Special Virus Leukemia Program. Also, prior to Dr. Stevenson's leaving, he had been involved in a program which I'll call the Solid Tumor Virus Program, which he was working on prior to his departure but about which I did not know a lot. I remember Dr. Rauscher calling me in and asking me what did I know about it, because whatever was there would be transferred into the Special Virus Leukemia Program. Also, Dr. Huebner had expressed an interest in moving contracts of his own Institute from NIAID over to NCI and expressed the interest in working on adenoviruses. And so all of that got transferred into the Special Virus Leukemia Program. And it was about at that time that Dr. Rauscher felt that it was time to change the name from the Special Virus Leukemia Program to the Special Virus Cancer Program because we were now going to take an interest in determining whether any viruses could be isolated from, or any evidence found, that the adenoviruses and any of the other well known viruses for which they had new antigens, the T antigens and antisera, could be found. Discussions had been held with Dr. Maurice Green to test the tumors for adenoviruses, *et cetera*. And everything was taking on a new approach. Do you want me to continue on, or do you want to go to the questions?

Baker: Let's go back to the questions and then we can pick up on some of this a little later. So, the first question, as you know, deals with the significant scientific results and who made those results in the period of 1950-1980.

Duff: Well, in reviewing this, I still feel that I like to start with Stewart and Eddy's work on the polyomavirus, because it was the first DNA tumor virus. And in the DNA tumor virus area I always think of Dennis Burkitt and the work on Burkitt's lymphoma; the isolation of the virus by Epstein and Barr; and all the work that the two Henles did, especially finding out that it was the causative agent of infectious mononucleosis; and the studies that Deinhardt did in the animals; and the work in Singapore and Hong Kong, and the only name that comes to my mind there is the work that Ho and others did on nasopharyngeal carcinoma. In regards to the mouse viruses, I would name Ludwig Gross and then, after that, comes a whole battery of Moloney; Rauscher; Rich; and Harvey's mouse leukemia viruses; Kaplan's radiation mouse leukemia virus; Friend leukemia virus; Kirsten's mouse sarcoma virus; and the Bittner agent for breast cancer in mice. Then the work that Carl Olson and Janice Miller did on leukemia in cows; the work of William Jarrett on feline leukemia virus; the work of Carr, Negroni and others at Cornell; and especially by Hoover and others at Ohio State on the feline leukemia vaccine studies; Murray Gardner on the feline sarcoma virus; Gordon Thelen on the wooly monkey; Kawakami on the simian sarcoma virus or from the Gibbon ape; and probably would end up with Robert Gallo on isolation of the first human retrovirus, the HTLV, or the human T cell leukemia virus, which I think is so important because it really was a precursor to the isolation of the HIV or AIDS virus. And I sort of left out all of the molecular studies by Huebner, Todaro, Temin and Baltimore with the reverse transcriptase, Spiegelman and Green, because that comes up again in question nine. But that would go, sort of, from Gross to Gallo, or from Stewart and Eddy to Gallo.

Baker: Well, that's a good list. I would add the later contributions of Bishop and Varmus and Weinberg on oncogenes. So, I think that covers the main characters all right. The second question. What do you think were the key administrative or management decisions affecting this field in this period of time and who made them, as you saw it, from where you sat?

Duff: Well, I came in '65, so everything was in place. The Special Virus Leukemia Program had been operating for a year. And the first big administrative change that I saw started in 1967 at the time of Dr. Stevenson's departure. That's when you became the Scientific Director for Etiology, Dr. Rauscher was the Associate Director, and Dr. Huebner was brought over from NIAID. I felt this was quite a change because that's when the program went from SVLP to SVCP (Special Virus Cancer Program). The work on the solid tumors was introduced. It was also at that time, as I recall, there was a budget cut. It was like the first time we experienced a budget cut. And I remember Dr. Rauscher saying that most of the money was going to have to come out of the Resource Program and that, of course, at the time, had been in Stevenson's bailiwick. And a number of the resources that the program had been collecting in the SVLP were now commercially available, things like collecting calf serum, horse serum, making specific cell lines, a number of different things were cut back or cut out completely because it was felt that they could be made commercially available. The contractors might need more money, but they could buy resources, and so on.

Baker: Do you think that would have been possible without the developmental research in the Resources Program earlier at NCI?

Duff: Probably not, but I think it also stimulated commercial development. For example, the sera that was collected by the program was of very high quality but, by that time, I think the commercial labs had improved and people were more confident in buying things commercially. Things that were not done at all when the SVLP started up were now beginning to be done by commercial labs. That didn't include everything, but it included a lot of it. And I think at that time--but I'm not sure when this all happened--but I think the Resource Program certainly was transferred out of the Viral Carcinogenesis Branch, the branch that Huebner was coming into, that Stevenson had left, and I think the Resources probably moved into the Office of the Associate Director. After Moloney came in as the Associate Director, they were definitely in that office. I felt that the National Cancer Program, and especially the acquisition of the Frederick Cancer Research Facility were important. I think that, of course, getting up to the point where the Zinder Committee came in, then a lot of changes began to be made. There were more advisory committees that came into being. Where the contracts had been in, for example, let's say, Dr. Huebner's office, they were all ultimately moved out of his office. I moved with the contracts. And all the contracts in the program were moved into what we called the Collaborative Research Branch, which was headed by Dr. Manaker. You had the Zinder Committee coming into play. You had a number of recommendations being made that the contracts should be terminated. And this ultimately led to the combination of the contracts, the grants and the resources under one branch, and all the research contracts were terminated and the contractors were advised, if possible, to apply for grants. And ultimately the resources and the services that were supplied by the Resources Group were changed. It was figured out whether a fee for service could be charged, and the contractor was requested to pay for some of the resources or the services that had been given to them free, to help pay for the cost of the contract.

Baker: Are you into the period after Rauscher left?

Duff: This went all the way up to about '83.

Baker: But when this change took place, the change from the use of research contracting moving more toward grants?

Duff: No. That didn't really occur until after the Zinder Committee, I would say, or about that time. That wouldn't have occurred until, I'd say, sometime around, between, '73 and '75.

Baker: Why do you think that took place?

Duff: Well, there was a number of reasons. I think when Dr. Huebner did come into the program, there was an expansion into a much higher, let's say, a different type of contract, a more multifaceted contract. Certainly, if we looked at the University of Southern California contract, you're talking about contracts in the million dollar magnitude. There were a lot of people, I think, who felt that the Huebner contracts were extensions of his own work. There were many people who felt that there weren't sufficient funds in the area of grants, and I think the outside people felt that some of the work that was going on in contracts was research oriented and that a lot of those people should be competing for grants. I think some of the people felt that maybe the contracts weren't getting as good a review as maybe the grants.

Baker: Do you think they did, or didn't?

Duff: I think they got a good review. Certainly a lot of the people who were either on, or are now on, study sections for grants were the same people who were doing the segment reviews in the Program..

Baker: Let me go back a little bit earlier, before you came. How much understanding and knowledge do you have of some of the key administrative decisions that were made to initiate the Special Viruses Leukemia Program?

Duff: Prior to my coming in 1965?

Baker: Yes.

Duff: My knowledge on that period is mainly what I have read. I came in '65 and I believe the Program had really started in '64, and so I would say it's either what I heard from people or what I read.

Baker: And so what is your impression of that?

Duff: My impression of it was that it was all very well planned. The two years that I really spent with the SVLP, before it became the SVCP, I thought it was all going along very well. But I saw a big change from the moment that--I'd say--from the time that Stevenson left and Huebner came into the program there was a big change.

Baker: Not necessarily worse or better, just different?

Duff: It was different. Right. Ultimately, Huebner had quite an operation and a number of people working together. I think he enjoyed seeing people work together. And there was a great deal of collaborative work, especially out on the West Coast. It ran almost from Seattle with the Helstroms, down to San Francisco with Ed Lynette and the California Public Health Service, the University of California, the University of California at San Francisco, Scripps Institute, the Salk Institute, and the University of Southern California. There was a whole group of people working together, and I think a lot of them were pulled together by Huebner.

Baker: For certain kinds of work you need this kind of collaboration and, as a matter of fact, the idea of the Special Viruses Leukemia Program was to bring together the different disciplines and coordinate the scientific substance with the resource allocations. And so, in a sense, this was not new philosophically, but it got to be perhaps much larger than people were used to seeing.

Duff: It was much larger than they were used to seeing and Huebner was a powerful person.

Baker: I would say "stimulating."

Duff: Yes. But I think he really saw that the cost of the resources, as the program changed, became such that people had to share them, and they had to know that Contractor A could collect tumors, but not everybody could collect them; otherwise it would become too expensive. Contractor B could grow some of the cell lines. And he wanted everybody to know what everybody could do and that everybody could, share and collaborate and work together.

Baker: Well, that sounds like a reasonable idea.

Duff: Right.

Baker: Okay. Let's go to the third question. What do you consider your main activities and effects of your participation in the program?

Duff: Well, from about the time Huebner came in for about the next 8 years-- When he came in, Dr. Rauscher set up a new segment in the SVCP called the Solid Tumor Virus Segment. All of the contracts that probably dealt with solid tumor research went into that area. The contracts that Dr. Huebner brought from NIAID, some that were left from the SVLP that Dr. Rauscher felt would fit in there, all went in there. Dr. Huebner was the Chairman of the Solid Tumor Virus Segment. I was the Vice-Chairman. And all of this was within the Viral Carcinogenesis Branch. And I'd say that was my biggest involvement, being sort of an administrative type person that was helping Dr. Huebner with the contracts that were under his bailiwick in the SVCP. This went until about 1975.

Baker: Okay. We've pretty well covered most of question four, which is who were the main leaders who influenced the direction and course of events. Anybody else come to mind who was influential in the direction and course of this area that you haven't mentioned?

Duff: Is this where the National Cancer Plan comes into play?

Baker: Well, we can cover that now if you wish.

Duff: Well, I just think that's a very important time frame because of the acquisition of the Frederick Cancer Research Center essentially for the Virus Cancer Program. It had been turned down by NIH. It was obvious that they didn't want it. And I don't think there were many people within the Cancer Institute that might have wanted Frederick, but I think primarily Dr. Rauscher saw its usefulness. And in this regard I think President Nixon made the big announcement about the National Cancer Program and the acquisition of Frederick, closing out the Biological Warfare Laboratories, and turning the guns into ploughshares, or however it was expressed. But, also, to me, it was a very interesting administrative shift, because a contractor, Litton Bionetics, was picked to run the Frederick Cancer Research Facility. Dr. Stevenson was the first Contract Manager of it, which was interesting to me, because I left Ft. Detrick to come to NCI, and he left NCI and ultimately ended up at Ft. Detrick.

Baker: Well, actually, the Ft. Detrick conversion was done before the National Cancer Program, and I really think so many political developments happened after the Ft. Detrick conversion that I don't see Ft. Detrick as really part of the National Cancer Program, but it became so later, of course. One person you haven't mentioned yet, who I consider a key decision-maker here, was Ken Endicott. But those decisions were made before you came, mostly, but critical on starting the Special Program.

Duff: Right. My only contact with Dr. Endicott was whenever we had to go before a committee that he would be chairing where we were perhaps explaining what we were doing in the area of contract research or resources. And so I had very limited contact with him. I'm not sure when he left.

Baker: '69.

Duff: '69? Okay. That was after Dr. Huebner was in.

Baker: Well, I went to run Etiology partly because we couldn't find anybody else to take it. And Ken Endicott was hopeful that I would ask Huebner to move to the Cancer Institute, but he didn't give me any resources to do it, so I had to carve out resources for Huebner, space in particular, but I thought it was worth it. So, I did it and I'm glad I did. He created a lot of problems, but he was worth it, I thought. Another person you didn't mention in the historical side was Ray Bryan, who certainly played a key role in the initial part of this, as well as keeping the flame of cancer virology alive when everybody thought viruses had absolutely nothing to do with cancer, which was the way it was before the special appropriations that were made in about 1964.

Duff: And I think Ray Bryan left the position of Associate Director when Rauscher came in.

Baker: Well, Ray didn't take to the administrative problems too well, so he was glad to get out from under some of that, and Rauscher took to them a lot better, so Rauscher, in a sense, replaced Ray Bryan as the initial head.

Duff: And so I guess my knowledge of a lot of these people is because that all transpired before I came.

Baker: Okay. Can you discuss, under question five, a little bit about membership on key NCI and NIH advisory committees and what do you think the main contributions of these committees were, and maybe some of the individual consultants that you recall?

Duff: Well, of course, I remember mainly the ones from the SVLP and the SVCP, and later on those within the Division. But, I suppose the people I know who were members of the Solid Tumor Virus Segment, for example Melnick, Hilleman, Sabin, Dulbecco, and Lynette, were all very much interested in the Cancer Program. I thought they made good contributions. They didn't always agree with Dr. Huebner, and they didn't always agree with his programs.

Baker: One should never expect virologists to all agree on much of anything.

Duff: Right. But it was very stimulating and, if everybody was still speaking to everybody by the end of a meeting, we were lucky. I thought that, for the most part, all the committees that I attended, the people were very much interested in giving us advice. And if they were reviewing contracts they did their very best to provide us with the input that we needed.

Baker: And, at this level, I think you would agree they were very helpful?

Duff: They were very helpful. Right.

Baker: Any lay members or political figures come to mind that maybe were influential in this program? Obviously, in terms of the National Cancer Program, there were key individuals. Early, Wendell Stanley was a person who was not directly involved in his research in this kind of biological aspect, but he was very helpful with Congress. And, I guess, Mary Lasker and Sidney Farber would come to mind.

Duff: Right.

Baker: Anybody else you recall there?

Duff: Of course, Dr. Sabin was, I think, influential. He was very much involved in the Program. He certainly was a well-known person.

Baker: On the chairmanships of the NCI staff committees, any comments on how that operation went?

Duff: The NCI Director's Executive Committee?

Baker: The internal committees reviewing the final decisions on contracts. So you had the Group Chairmen formed one group that provided a key secondary review and, of course, the NCI had what later was called the Executive Committee or the Scientific Directorate, which had final approval of recommendations to Endicott. Do you have any recollections of any of those activities that you might comment on? Other than the time when a grant from Linus Pauling would come up on whether vitamin C had any effect on cancer. I think that was about the most dramatic experience you could sit through up there.

Baker: One other scientist who was involved in the committee chairmanships that I found helpful was Chuck Evans from the University of Washington. Did you ever have any linkages with him?

Duff: No, I didn't. But I do feel that some of the lay people were very good, especially the local lady here--I've forgotten her name--Kushner, I believe, was her name.

Baker: Rose?

Duff: Rose Kushner. And she was involved in breast cancer. Extremely good.

Baker: Okay. We turn now, in Question Six, to a different area, and that's the value, or lack thereof, of resources themselves, such as antibody preparations, virus preparations, tissue culture cell lines, special animals. How do you feel the availability of these materials that were made possible through the Program affected the course of research and so on?

Duff: Well, I always thought that, right from the beginning, that they were all quite valuable. And they certainly, I think, made quite a contribution to the entire Program, especially in products like reverse transcriptase and various amounts of viruses that people were using for molecular studies that they would never have been able to do this work without, perhaps, the ability for the Program to supply them to them. The cell lines and the cell banking and the identification of various cell lines that were done out at NBL (Naval Biological Laboratory) and up in Michigan were extremely valuable to people. They just didn't have the wherewithal to do that.

Baker: Some of the old line guard outstanding scientists felt that commercial companies could not produce sufficient quality on viruses and antibodies and so on. So, one of the important functions, of course, of your activities was to ensure quality. And we finally, of course, convinced people that the quality could be just as good produced by contractors as by university scientists and, not only that, but the quantities available were important. I sort of joked with some of the virologists that they were very good about exchanging samples when they isolated viruses or made preparations but, by the time they distributed samples to their colleagues for testing they didn't have any left to work with. And I knew we were in good when Moloney, one day, came in and said, "We just got the new virus preparation from Pfizer and it's as good as anything we ever produced and they've got buckets full of it." So, I think that was a hump that took a little doing to get over. You probably arrived when that problem was almost solved?

Duff: Right. But then it all took on a new look. There was always a continuous need throughout the whole time of the Program for resources, right up to the very end. And then people began to feel that perhaps contractors ought to be paying for some of this, but again that's at a stage where I think people were still trying to convert contract money to grant money, and I imagine to this day that the Resource Program is still going on.

Baker: Well, this Question Number Six also asks you to comment on the availability of such resources in 1955 compared to 1975.

Duff: Well, I'd say, since I wasn't around there in '55, that I--

Baker: Well, you were in the field, so--

Duff: Yes. They probably were not readily available. My recollection, when I came, is that there was a group in NIAID who was preparing mainly antibodies or antisera for the identification of organisms and those were available to a limited degree. But there really wasn't much available, I don't think, around '55.

Baker: Even tissue culture was sort of an art form that involved some mysticism still.

Duff: Right.

Baker: But that field has grown magnificently with all the things that are done with tissue culture now. So I think this also laid some groundwork for biotechnology, as well as other things.

Duff: Oh, yes.

Baker: Do you have any appreciation of the relative funding of grants compared with contracts when you were there?

Duff: I really was not aware of what the funding was in the grants area until the grants were transferred over to us. And until they were actually transferred and we knew what we had on paper, I think we were really unaware of what was in the grants area.

Baker: So, do you recall what that comparison was at that time?

Duff: I would have to go back. I don't have any--

Baker: I mean, were they about the same?

Duff: It was lower. It was certainly--

Baker: The grants?

Duff: The grants were certainly. The total funding of grants was certainly lower than when the SVCP was at its peak. I would say that the contracts might have gotten to be almost twice the amount as it was in grants, but that would be at its highest.

Baker: Do you think that was good or bad?

Duff: Well, I think that I would say that compared to what we were doing--and that also included resources and everything--it was understandable to me. I don't think there was anything wrong with it.

Baker: Well, here's another tricky question, number eight, on if you could have changed anything, as you look back, what would you have done differently?

Duff: Well, I gave that a lot of thought and I know that there was criticism of Dr. Huebner, there was criticism within NIH. I remember one time Leon Jacobs pointing out to me that he felt it was wrong the way NCI was using its contract funds to fund research which he thought could be conceived to be extensions of a person's intramural research. Many scientists weren't even comfortable seeing so much money being spent on that part of the Program. That program reminds me more of the Special Virus Cancer Program. Sometimes the scientists weren't comfortable with the amount of money and the size of the cost of projects, and I see a lot of similarities between the two. And it's also interesting to me because James Watson was very instrumental in our Program at the time the Zinder Committee was meeting and the Program was winding down and we were talking about large contract type programs, and he became very much involved in the Human Genome Project.

Baker: Oh, yes. And I think he saw you couldn't really tackle that with $30,000-$50,000 dollar grants which is, to me, obvious.

Duff: Right. I like to look at those two programs because I see a lot of similarities between what was started up in, like, say, in the Cancer Program. I think the AIDS Program really benefited from having all these trained scientists come in. AIDS research would never have moved the way it did without them.

Baker: The public is sympathetic to science when it has some hope of a solution to serious problems. How sympathetic are they to science in general these days compared to then? And my question was really also getting at their understanding. Is the public's knowledge of science--and I don't mean just biomedicine--better, worse, or the same as it was in 1955?

Duff: I think it's better. I think people can better understand. They certainly can understand, I think, AIDS. I think people are even able to talk about gene therapy. At least they read about it in the newspapers and they know how to mention the word.

Baker: You're not concerned about the books of innumeracy and lack of knowledge of science by so many people, about having no grasp of the Thermodynamics Second Law, and what not?

Duff: Well, yes. Whether the interest in science is falling off, that I don't know, but I keep thinking that there is so much going on in the field of the human genome and AIDS research that people, I keep feeling people are becoming better educated.

Baker: Certainly those who want to learn, there is available information now that is better. *The Washington Post* on Mondays, I think, is doing a rather good job on educating laymen about science. However, I was teaching general science to non-science majors, because I don't think by and large those who are not going into science get adequate grasp of the fundamentals of science, and I believe that's partly because the science departments are so interested in teaching others who are interested in science that they don't spend much time on those who are not interested in going into science. And then they wonder why the public hasn't been more understanding of some of the more esoteric areas of science is the way I see it. But, I guess you don't necessarily feel that way.Any additional comments, or any statements, or disagreements, or objections that you have?

Duff: No. There was one thing interesting to me, and that was shortly after Dr. DeVita became the Director of NCI, it was my understanding that *The Washington Post* was, I believe, under the direction of Woodward, going to write an expose on the Cancer Program and it was not going to be a favorable article. Anyhow, this young man comes to my office to talk to me about the Virus Cancer Program, and the first comment he made was, "It's my opinion that the Virus Cancer Program should never have been started to begin with."

Baker: This was a reporter?

Duff: This was a reporter.

Baker: Did you ask him why he had that opinion?

Duff: Well, I guess his whole approach was going to--

Baker: Yes. He wanted to fire you up probably.

Duff: Right. I've never even thought that it shouldn't have been started. And I thought, well, you know, he's a very young person. He probably, you know, I'm visualizing this as maybe '75, 1975, and he couldn't have been much more than in his twenties, and I thought, well he had never lived back in the age of polio and at a time when you saw the dreaded disease, and you could see how a vaccine brought it all to a screeching halt, and how it would really be very natural for scientists to want to tackle a project such as cancer, and information that was coming along in the field probably, in the '60s.

Baker: Well, there are plenty of older scientists, academic scientists, who would probably say the same thing he said.

Duff: Probably so.

Baker: And Dr. Mider was never sold on the whole idea of the programs that utilized contracts, and Leon Jacobs, earlier, I think, felt the same, although I'm not sure he hasn't changed his mind by now, that maybe it's not all bad.

Duff: I'm not sure.

Baker: But it's an ongoing argument on whether the money is better spent one way or the other, and from my view, as Director, it wasn't a question of "either/or," you needed both. Contracts could be used for coordinating very different things that needed to be coordinated but, for exploratory research, you certainly didn't want centralized decision-making, and so the grant system was excellent for that. But, to say it ought to be one way or the other always seemed rather stupid to me, and I worked both in the grants area and in the intramural areas, on the outside in private industry, and in the Ludwig Institute operated with private funding, and all of these things are not that different. You need different things for different problems. And I always felt that too many of the academic scientists didn't have an appreciation of the value of things beyond the grants and the old-fashioned intramural area. And it's not, I think, as I say, "either/or," but we need to do both. If you start out trying to tackle cancer, then you'd better think through what ought to be done, which brings me to one final question. How did the systems planning efforts look to you and the effects of that since, as you know, there are many scientists that think there shouldn't be any planning because it interferes with their individual decisions of what should be done next?

Duff: Well, I would be the type of person who feels, when I saw all the planning, especially all the planning that was done in the early times, that it would be very worthwhile. I was all for it. And a lot of the things we did, in my mind, in the first two years I was with Stevenson, worked out in that way. I then began to find out that even people who were maybe in my own section within NCI were very grant oriented. Their minds are not to be organized or to follow a flowchart. It just does something to them. But I still think it is valuable.

Baker: Well, I understand this because when I was in the laboratory I had the same perspective. But, as I got more and more involved in broader aspects, a more programmatic perspective, it became clear to me that that view of isolated and relatively small projects was not going to tackle a lot of problems that needed tending to. And so the earlier view I had and the common view among academic scientists isn't wrong; it's just incomplete when you look at the higher organismic level of things. And so I turned to planning primarily, originally, to help in deciding budget submittals.

Duff: Yes.

Baker: How do you convey to the Congress what you want to do with this money you're asking for? And there are plenty of Congressmen that weren't too satisfied with the answer, "Well, we give it to the individual investigator to do what he wants to do." While I would agree historically a lot of great things happened with that, again it's not all there is to it. So it's an interesting ongoing conflict which is probably never going to be resolved really.

Duff: Probably not. And especially if you're involved with a lot of resources and the utilization of resources I think you can see the need for the planning. But I did find quickly that the bench scientist was turned off.

Baker: Oh, yes.

Duff: They're happier with grants, a donation of money and let them do what they want.

Baker: Well, there aren't many groups in society that have been able to do that. And, of course, it is true that the percentage of the recommended approved proposals that are being paid now is rather low, and that's a matter of how much this nation wants to invest in the total amount of money. So, it's amusing to me that the complaints about not enough money are just as loud now, when the Cancer Institute's budget is $1.2 billion, approximately, and the complaints are just as loud, if not louder, than they were back when it was $430 million dollars, which is what I testified for right before I left. So, as Endicott used to say, "It's a bottomless pit."

Duff: Yes. I think it is.

Baker: Well, I certainly thank you very much for your willingness to do this and I see you've given it some thought, so I appreciate it very much.

Duff: Oh, sure. You can even have my notes if you want them.

Baker: Well, I might take them.

Duff: All right.

Whereupon, the interview concludes